allowance to be made for its fluctuation. It is well known that thermoelements deteriorate at high temperatures. This results in an incorrect reading and the error depends on the distribution of temperature in the furnace and, therefore, on the amount and nature of the charge which is being examined, etc. Trouble from this source was largely removed by comparing the working elements with standards which were used for so short a time as to hold their values practically unchanged for several months. The comparison must be made under exactly the conditions for which the temperature reading is intended. Thus for best results in the determination of melting points, comparison must be made during the melting. The relative error of a temperature measurement below 1,550° can in this way be brought within half a degree.

One conclusion from the work is that the temperature of lavas where wollastonite is found can not have exceeded 1,163°.

Charles K. Wead,
Secretary.

DISCUSSION AND CORRESPONDENCE.

DR. O. F COOK'S 'SOCIAL ORGANIZATION AND BREEDING HABITS OF THE COTTON-PROTECTING KELEP OF GUATEMALA.'

Some of the results of the continued work of the United States Department of Agriculture on the ponerine ant, *Ectatomma tuberculatum* Olivier, introduced into Texas for the purpose of aiding in the extermination of the cotton boll weevil, are given in this paper of fifty-five pages in advance of an illustrated bulletin or report on the same subject. Dr. Cook's paper can hardly be passed over without comment, since it displays so many misstatements of fact, such inadequate knowledge of the work that has been done on other species of ants, and such a wilderness of unkempt argument and speculation as to entitle it to high rank as an example of what a scientific essay should not be.

The burden or 'Leitmotiv' of the whole paper is properly sounded in the introduction, which is well worth quoting in its entirety:

In preceding reports treating of the kelep as an enemy of the cotton boll weevil the distinctness of its behavior from that of the true ants has been noted. To avoid in some measure the misapprehension likely to be caused by calling it an ant it seemed desirable to introduce with the insect its distinctive Indian name, *kelep*. In the minds of the natives of Guatemala, the kelep is not a kind of ant, but an independent animal not to be associated with ants. The more we learn about it the more this aboriginal opinion appears justified, not alone because the kelep is a beneficial insect, but because it has a different mode of existence and a different place in the economy of nature.

The popular classification of the social hymenoptera recognizes three types—the ants, the bees and the wasps, the ants being distinguished from the others by the absence of wings. The kelep falls, however, into none of these groups. To call it a wasp or a bee would not misrepresent the practical facts more than to call it an ant. In reality the kelep represents a fourth category of social hymenoptera, as distinct from the other three as they are from each other. Authorities on the classification of the hymenoptera have admitted a rather close affinity between the wasps and the ants, but the kelep differs from both of these groups and approaches the bees in important respects, and especially in those which affect the question of its domestication and utilization in agriculture.

It was naturally supposed at first that the kelep would have the same habits as the true ants which have been associated with it as members of the same family or subfamily, but the differences were greatly underestimated. If the hymenoptera were classified by a taxonomic system consistent with that applied to the higher animals, the kelep would need to be recognized as the type of a new and distinct family. It is, moreover, the first member of its family of which the habits have become known. Under such circumstances it was quite impossible, obviously, to determine in advance whether its habits and instincts would permit its colonization in the United States and its use in agriculture.

The fundamental difference between the ants and the kelep, and that in which the latter resembles the honey bee, lies in the methods of swarming. Among the bees and the keleps swarming results directly in the formation of new colonies, but the swarming of the ants is a distinct biological phenomenon having for its object cross-fertilization. The kelep is completely socialized, like the honey bee, while the ant is not. The keleps and the honey bees live only in communities, while the ants at one stage of their life

history leave the nest and meet the vicissitudes of independent existence as solitary individuals, like the non-social insects. The social organization of the kelep represents a line of development distinct from that of the ants, and shows a relationship with the parasitic and predaceous wasps rather than with the true ants.

Although fresh surprises meet the reader at every turn as he proceeds to read the paper through, he is led to suspect that Dr. Cook, in spite of his fluent style, may at times be unable to say exactly what he means. He evidently wishes to make us believe that the kelep *quâ* dried insect, spitted on a pin, is nothing but a poor ponerine ant, but *quâ* living, nest-building, boll weevil exterminator, is really a creature *sui generis* which the advanced systematist would do well to regard as the sole representative of a distinct family, the Kelepidæ. Here Dr. Cook shows admirable self-restraint, for it might just as well be made the type of a new phylum (Kelepata) or subkingdom (Kelepozoa). At any rate, it is clear that the kelep rises to a dignity analogous to man, whom certain theological taxonomists regard as a poor, though upright primate physically, but as belonging psychically to an entirely different order of being, because he is possessed of the 'free intelligence of the angels.'

Dr. Cook's amazing estimate is attributable to a confusion of ideas concerning certain well-known phenomena among social insects in general and to a lot of inconclusive, not to say slovenly, observations on the kelep in particular. He begins by confounding the nuptial, or marriage, flight and the swarm, or, at any rate, by continually introducing these in his discussion where they do not belong. The nuptial flight is a well-known occurrence in all social insects that have winged males and winged females, in the honey-bees as well as in the ants and termites. Nevertheless, Dr. Cook believes that it is sorely in need of a new name and suggests 'concourse,' a designation as superfluous as it is inept. Swarming, on the other hand, which is peculiar to the honey-bee, is characterized by the old queen leaving the hive with a detachment of workers and establishing a new colony, while the young queen takes her place with the remaining workers. When he comes to consider the possible occurrence of this phenomenon in the kelep, Dr. Cook increases the confusion by failing to distinguish sharply between 'nest' and 'colony.' A single colony of ants may be confined to a single nest, in which case it has been called monodomous by Forel, or it may extend over several nests, in which case it is polydomous. The latter may have several queens distributed among the different nests. The workers of these are on friendly terms with one another and may visit back and forth. Undoubtedly the inhabitants of such nests occasionally become detached from the parent colony and may be regarded as new colonies formed by a process of budding or stolonization. These conditions are well known in such highly endowed ants as our species of *Formica* and *Camponotus* (*F. rufa, sanguinea, exsectoides, C. maculatus* var. *sansabeanus,* etc.). While there is an unmistakable resemblance between this method of colony formation and the swarming of bees, these ants retain in addition the primitive method of founding colonies by single deälated queens.

Now Dr. Cook's confusion of ideas and lack of information are most flagrantly displayed when he comes to present the facts that seem to him to warrant the separation of the kelep from the true ants and ally it with the honey-bees. Having made the interesting observation that a kelep colony will form new nests by sending out detachments of workers and females or of workers alone, he shuts his eyes to the resemblance between these conditions and those of the higher ants, and forthwith jumps to the conclusion that the kelep can not be a true ant, but must be at least as closely related to the honey-bee.[1] Obviously the very opposite is true, since his observations, rightly interpreted, show a closer relationship between the kelep and the higher ants than has been supposed to exist among

[1] "Kelep nests are frequently placed only a few inches apart, the workers of different colonies not being actively hostile. Members of two colonies will forage on the same cotton plant or tree trunk with no signs of animosity" (p. 14).

the Ponerinæ. But this is not all. Because he has never seen a nuptial flight of male and female keleps, he jumps to the further conclusion that it never occurs and that colonies of this ant can not be founded by solitary females. He says at p. 34, 'there is no provision in nature for a solitary kelep.' His whole description of the nesting habits of the kelep discloses nothing to warrant such a gratuitous assumption. As the colonies are small, their nuptial flights would hardly be noticed by the Indians of Guatemala and may, moreover, occur only during certain years or in the twilight or after dark. That they have not been seen in the colonies brought to Texas is even less surprising, as such flights among other species are celebrated only by flourishing colonies, and everything goes to show that Dr. Cook's importations are not in that condition. The large number of males which he finds suggests a high degree of fertility on the part of the workers. It does not, however, indicate colonial prosperity in these ants, but a scarcity of females. Very similar conditions have been observed by Miss Holliday[2] and myself in another ponerine ant, *Pachycondyla harpax* of Texas, which does not form polydomous colonies.

It is, of course, possible that the nuptial flight may not occur in the kelep, that the males may wander about and fertilize the females within the nests, and that new colonies may be formed exclusively by a process of budding or subdivision of preexisting colonies. But if this is true, we should be led to inferences very different from those announced by Dr. Cook. Far from having 'complete socialization' and representing a higher and more economical form of social life, the kelep would seem to be a retrograde, degenerate or, at any rate, highly specialized ant for the reason that just such conditions, at least so far as the suppression of the nuptial flight and intranidal mating are concerned, occur, in all probability, among the parasitic ants like *Anergates, Symmyrmica, Formicoxenus,* etc., and in highly specialized ants like the Dorylinæ and *Leptogenys,* which are either rare or have an unusual mode of life. And far from being a promising trait in an ant introduced for economic purposes, the very opposite would be the case, as seems to be indicated by the flat failure of Dr. Cook's propaganda. It may be best, however, to refrain from all speculation on this matter till we know more about the colonizing habits of the kelep than can be learned from Dr. Cook's desultory statements. There can be no doubt about the fact that isolated fertile females of certain Ponerinæ are able to establish colonies. In the Bahamas I found satisfactory evidence of this both in *Pseudoponera stigma* and in *Odontomachus insularis,* and Dr. Cook is still a long way from having proved that the same method is never adopted by *Ectatomma.*

Additional confusion is introduced by Dr. Cook with a set of new terms. He calls 'an insect colony in which all the eggs are furnished by a single laying queen' a 'strictly determinate organization, that is, it reaches a natural limit after the mother insect dies or ceases to reproduce,' and 'colonies may be called indeterminate when the social economy of the insect is such that a lost queen can be replaced.' "Colonies with more than one egg-producing queen may be called compound indeterminate." All of these distinctions are at the present time not only superfluous, but misleading. According to prevailing theory, all ant, wasp and honey-bee colonies would be determinate, since it is supposed that they can not produce females after the reproductive exhaustion or death of the queen. And, for aught we know to the contrary, the same may be true of the termites. Until we are sure that this is not the case, we gain nothing but confusion by adopting such a classification.

Equally futile is his distinction between the 'social principle of matriarchy' and 'ergatarchy' among the social insects. As a member of a colony, the female ant, wasp or humble-bee is no more a ruler or dominating factor in social life than the queen honey-bee. If the female ant, wasp and humble-bee display great initiative in founding their respective colonies, the female honey-bee displays it by killing rival queens, returning to the hive after the nuptial flight, etc.

[2] 'A Study of Some Ergatogynic Ants,' *Zool. Jahrb. Abth. f. Syst.*, XIX., 4, 1903, p. 297, 298.

The following remarks quoted at random from Dr. Cook's paper show the care with which he has studied the literature of his subject. At p. 9 (foot-note) he says:

With these fungus-cultivating ants and termites, at least, it would seem that a new colony can scarcely be founded by a pair of sexual termites or by a single fecundated female ant unless they carry their domesticated fungus with them. It is possible, however, that in both cases the newly mated insects are adopted and set up in housekeeping and farming by workers of their own species, who bring 'spawn' of the fungi from the older colony with which they are in communication. This might the more readily happen because long subterranean galleries are a prominent feature of the architecture of the fungus-growing insects, both ants and termites.

Although nothing is known concerning the origin of the fungus gardens among termites, von Ihering, in an article[3] which should be known to every botanist, has shown that the colonies of *Atta sexdens* are established by isolated queens and how these insects carry over the fungus from the maternal nest to their own. These observations have been fully confirmed by Goeldi[4] and Huber.[5] At p. 24, Dr. Cook says: 'Copulation has never been observed among termites.' On the contrary, it has been repeatedly observed by at least one observer, Dr. Harold Heath.[6] At p. 19 we find the statement that in '*Leptogenys*, the females, though wingless, are very different from the workers.' Miss Holliday and myself have shown in three different papers that the females of this ant can be distinguished from the workers only by a difference in the size of the abdomen and the enclosed ovaries. At p. 17 we find the following statement:

It does not appear that the keleps have the art of regurgitating food for their larvæ or for each other, but they have, instead, the curious habit of opening their mandibles wide and lapping up drops of nectar, moistened sugar or honey on their mouth-parts. The liquid is thus carried into the nest and dispensed to the other members of the community, old and young. The queen is regularly fed in this way, though in a few instances, the queens of captive colonies came to the surface to eat sugar with the workers.

The mode of expression is varied to read as follows at p. 42:

The kelep does not appear to have the art of regurgitating food as do the true ants, but it is the regular custom of the workers to gather up on their mouth parts large drops of nectar, syrup or honey, which are carried into the nest and freely dispensed to the remaining members of the community, as well as to the queen and larvæ.

To any one familiar with the structure of the mouth-parts of the kelep and with the behavior of ants while they are feeding one another, these statements can only mean that the kelep, like the higher ants, not only ingurgitates liquid food, but feeds the other members of the colony by regurgitation. Here, again, Dr. Cook makes a botch of an interesting observation in his desire to make the kelep out to be a most exceptional creature.

In another part of the paper he shows that this ant also feeds its young with pieces of insect food in exactly the same manner as I have described for other Ponerinæ and some of the higher ants (*Aphænogaster, Pheidole*), and as Janet has shown for *Lasius* and Adlerz for *Tomognathus*. Instead of drawing the natural conclusion that the kelep is allied to both the Ponerinæ and higher ants, Dr. Cook concludes that its relationships are 'with the parasitic wasps rather than with the ants.' It is evident that he will be satisfied with any relationship except the true one. As a matter of fact, every habit which he describes shows that the kelep is nothing more nor less than a ponerine ant. It differs from the Ponerinæ hitherto studied and approaches the higher

[3] 'Die Anlage neuer Colonien und Pilzgärten bei Atta sexdens,' *Zool. Anzeig.*, Bd. 21, 1898, pp. 238–245, 1 fig.

[4] Forel, 'Einige Biologische Beobachtungen des Herrn, Prof. Dr. Goeldi an brazilianischen Ameisen,' *Biolog. Centralbl.*, XXV., 1905, pp. 170–181.

[5] 'Ueber die Koloniengründung bei Atta sexdens,' *Biolog. Centralbl.*, XXV., 1905, pp. 606–619, 625–635, 26 figs.

[6] 'The Habits of California Termites,' *Biol. Bull.*, IV., 2, December, 1902, p. 52.

[7] *Loco citato*, pp. 295–297. 'A Study of some Texan Ponerinæ,' *Biol. Bull.*, II., October, 1900, p. 7; and 'A Crustacean-eating Ant (*Leptogenys elongata* Buckley),' *Biol. Bull.*, VI., 1904, p. 251.

ants in having the power of feeding by regurgitation and of forming polydomous colonies. These conditions merely serve to link the Ponerinæ more closely with the Myrmicinæ, Camponotinæ and Dolichoderinæ. Dr. Cook destroys the value of his own observations by continually using them in support of his perverse speculations. I can see no reason, therefore, for revising my opinion in regard to the taxonomic and economic status of the kelep as expressed in two previous papers in this periodical.[8] Apparently the harder Dr. Cook works to confer exceptional attributes on the kelep, the greater becomes its similarity to other ants, especially to the relatively unplastic Ponerinæ, and hence the less promising it becomes as a subject for agricultural experiment.

The sole result, which, in my opinion, we had a right to look forward to, from all this Corybantic enthusiasm over the introduction of an exotic ant into the United States, was not the protection of the cotton plant from the attacks of the boll weevil, but the production by some well-trained entomologist of a carefully written and illustrated memoir on the structure and habits of a ponerine ant. Under the circumstances and with the funds and facilities at its disposal, this lay well within the competence of the Bureau of Entomology, and may, in fact, be actually under way in the promised report. But assuredly Dr. Howard is not to be congratulated on the kelep articles hitherto published under the auspices of his bureau. We are accustomed to receiving much better work from that quarter.

William Morton Wheeler.

ISOLATION AND THE ORIGIN OF SPECIES.

President Jordan's paper 'The Origin of Species through Isolation'[1] has been read by me with much interest. The following paragraph may be quoted as the caption under which he writes:

> In nature a closely related distinct species is not often found quite side by side with the old. It is simply next to it, geographically or geologically speaking, and the degree of distinction almost always bears a relation to the importance or the permanence of the barrier separating the supposed new stock from the parent stock.

It appears to me, however, that the case as stated by him can find scant support of the botanists, to whom it is, I think, easier to find exceptions to the rule, than facts in support of it. The question is, of course, a very complicated one and all who embark on a discussion would fain sound the '*obligato*' of Leonard Stejneger, 'so far as I know.' A few instances drawn at random will suffice at least to throw a reasonable but large doubt upon the factor of isolation and the extent of its effects, as stated by President Jordan, at least so far as plants are concerned, and this doubt should, I believe, obligate us to put the caption cited above into the form of an open question.

Lycopodium complanatum L. and *L. tristachyum* Pursh are two very distinct but closely related species of club-mosses occupying the same range. If we attempt to construct a theory of their origin we are compelled to regard them as genetically related, whatever the mode of origin may have been. These species often grow intermingled in the same habitat, and it was the contrast which they presented under such conditions which forced me to examine them with great care and finally to decide upon their distinction.[2] And if, as has been urged, our eastern North American plant, *L. complanatum,* is not the true European species, the case is strengthened rather than weakened.

In the deserts of the southwest are to be found numerous closely related species of cacti, especially of the genus *Opuntia,* occupying the same habitats and, perhaps, the same ranges. It would be difficult to apply the principle of isolation to these. As an example I may say that there are two distinct but closely related species of the prickly pear type, which I may not, in the present state of their taxonomy, presume to name, distinguishable by their fruits, which are in one species

[8] Science, September 30 and December 2, 1904.

[1] Science, II., 22: 545–562, November 3, 1905.

[2] Lloyd, F. E., 'Two Hitherto Confused Species of *Lycopodium*.' *Bull. Torrey Botan. Club,* **26**: 559–567, November 15, 1899.